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## ***Interactive comment on “A two-habit model for the microphysical and optical properties of ice clouds” by C. Liu et al.***

**C. Liu et al.**

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Responses to Reviewer #1 (ACP MS No.: acp-2014-441)

We would like to thank the reviewer for the valuable comments and constructive suggestions. In the revised manuscript, we have accommodated all the suggested changes.

Anonymous Referee #1

General comments The paper presents a two-habit mixture model to represent the general microphysical, optical and radiative properties of cirrus. The authors base their two-habit mixture model on recent observations that show ice crystals evolving

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from simple compact shapes to more spatial aggregated shapes with increasing ice crystal maximum dimension (observations that are generally shown throughout the literature as well as aggregation simulation studies). The habit mixture can be changed continuously across the PSD without discontinuities. The authors use the latest light scattering methods to compute the single scattering properties of their model and include both surface roughness and hollow hexagonal cavities. They make extensive use of in situ, laboratory, and satellite remote sensing measurements, inclusive of polarization, to show that some form of habit mixture is required to consistently simulate the observations from across the spectrum and as a function of scattering angle. A single hexagonal column model is shown not to simulate the same observations to the same degree as the habit mixture model. The results presented are worthy of publication and the paper is thorough and understandable. Although their model consists of only two habits, the second habit is composed of 20 hexagonal monomers, this leads to the following questions and points that the authors need to address before the paper can progress to ACP.

The points and questions below are not considered major but need to be addressed to improve the paper.

Points to consider 1. The aggregate model consists of 20 monomers, why 20? Why not 10, 15, 18? Please justify why 20 has to be used. Is it the case that to satisfy the measurements of Dmm and IWC this many monomers is required? From light scattering calculations it is shown in Figures 5 and 6 of Baran (2009) that adding hexagonal monomers beyond 3 components does not significantly change the phase function (asymmetry parameter) due to the aggregates being spatial, i.e., multiple scattering between monomers is not significant. Indeed, the g-values are shown to asymptote. The results contained in Baran (2009) are based on an ice aggregation model developed by Westbrook et al. (2004). In the case of the aggregate model proposed by the authors how many monomers are required for the phase function (asymmetry parameter) to asymptote?

Response: Although Baran (2009) demonstrated that adding hexagonal monomers with the element number beyond 3 does not significantly alter the asymmetry factor, in this study we select 20 monomers for three reasons: 1) as an appropriate particle geometry is sought to mimic the complicated morphologies of realistic aggregates within ice clouds and it seems to be an oversimplification if only a few monomers are used; 2) an aggregate geometry corresponding to a potentially lowest value of the asymmetry factor is desired, and it is found that the asymmetry factor slightly decreases as the number of monomers increases; 3) with the trial and error method, the use of 20 monomers is optimal in terms of the balance between the computational effort in light scattering simulation and the performance of the particle habit model in fitting the measured microphysical properties (specifically, IWC and D<sub>mm</sub>).

2. In the construction of the aggregate model are intersecting planes avoided? This should be stated.

Response: The criteria used by Xie et al. (2011) to detect overlapping of two hexagonal particles are used to avoid intersecting particle faces in this study, as explained in the revised manuscript.

3. Please could the authors state the orientation-averaged area ratio and fractal dimension of their aggregate model in the paper and how do these compare to observation?

Response: We did not consider the orientation-averaged area ratio or fractal dimension of the model. However, the two parameters are very important for determining particle geometries, and have been investigated by other researchers using available observations.

To give more information about the geometry of the aggregate used in this study, we added the geometric parameters, volume and projected area of the aggregate in the revised manuscript (please see Appendix A and Table A1).

4. In the appendix please also include the full co-ordinate (x,y,z) geometry of the

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hexagonal aggregate model and are the aspect ratios of each monomer kept constant at a value of 1?

Response: The parameters used to fully determine the aggregate as well as its volume and projected area are added in the appendix. As we explain in the manuscript, the aspect ratios are randomly chosen between 0.8 and 1.

5. The definitions of maximum dimension between the SCM and ice aggregate are not the same. If definitions were the same what effect would this have on your calculations when comparing properties of the same maximum dimension? This will not fundamentally alter their conclusions but will impact their calculations to some degree, the question is how important is it? Is the definition of maximum dimension applied to the aggregate robust under different viewing geometries?

Response: It is true that the definition of maximum dimension does not fundamentally affect our conclusions. For the aggregate, the maximum dimension is defined as the maximum distance of two points on the aggregate element surfaces, and it is independent of particle orientation.

6. I dispute the use of the term “spectral consistency”. What is shown in the paper is that the two component model is more consistent between 5 wavelengths, and the wavelengths are composite band-averages rather than monochromatic differences. To be truly spectrally consistent the authors need to show that the model is monochromatically consistent across high-resolution radiance spectra spanning the visible, near-ir and long-wave regions as demonstrated by Baran and Francis (2004). At the moment, the authors may only state that their model fits composite band-averaged measurements comprising of five wavelengths.

Response: The use of the term “spectral consistency” may overstate the advantage of the two habit model. However, we clearly explained the term in the manuscript, and a reader should understand the meaning of “spectral consistency” within context. So we prefer to keep the term.

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7. Figure 4. Could the authors be more quantitative? especially when measurements of IWC are over many orders of magnitude. I suggest plotting PDFs of measurements and model results over intervals of Dmm and IWC and using a statistical method to quantify the goodness of fit?

Response: The histograms of the distributions of the measured and calculated Dmm and IWC are given (see the lower panels of Fig. 4), and, as expected, both the Dmm and IWC distributions given by the THM closely agree with those of the measurements. Furthermore, we illustrate the mean relative differences and their standard deviations of the theoretical microphysical properties at different bins of Dmm and IWC. More detailed discussions of the comparison between the theoretical and measured microphysical properties are added in the revised manuscript. In addition, a new table (Table 1) and a new figure (Fig. 5) have been added.

8. Figure 6. These are bulk comparisons. The authors employ a number of different light scattering methods to compute the scalar optical properties as a function of D. I would also like to see a figure showing a plot of the scalar optical properties as a function of maximum dimension to show that there are no discontinuities occurring between the different light scattering methods.

Response: Following the suggestion, we added a new figure (Fig. 7) for the extinction efficiency, single-scattering albedo and asymmetry factor of the SCM and THM as functions of D. It can be seen that there are no noticeable discontinuities in the results.

9. The paper does not at all discuss how cloud vertically inhomogeneity and 3D cloud effects may impact their results. Some discussion of these effects is warranted, especially with regard to the more recent study by Fauchez et al. (2014), found here <http://www.atmos-chem-phys.net/14/5599/2014/acp-14-5599-2014.pdf>

Response: This study does not address issues related cloud vertical inhomogeneity and 3-dimensional radiative effects. This point is clearly stated and the recommended reference (Fauchez et al., 2014) is cited in the revised manuscript.

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## Minor points and typos

1. Page 19547 line 10, “an ensemble habits”-> “an ensemble of habits..”

Response: corrected. Thanks for pointing out the typo.

2. Page 19547 line 15 consider adding this citation Baran et al. (2014), as the paper demonstrates the importance of constraining habit mixture models and PSDs, assumptions regarding the former and latter are shown to significantly affect climate model calculations of SW and LW fluxes at TOA (Baran, A., P. Hill, K. Furtado, P. Field, and J. Manners, 2014: A Coupled Cloud Physics-Radiation Parameterization of the Bulk Optical Properties of Cirrus and its Impact on the Met Office Unified Model Global Atmosphere 5.0 Configuration. J. Climate. doi:10.1175/JCLI-D-13-00700.1, in press.)

Response: This is a relevant paper, and is now cited in the revised manuscript.

3. Page 19548. In the discussion of surface roughness a citation to Ulanowski et al. (2014) should also be added, which can be found here <http://www.atmos-chemphys.net/14/1649/2014/acp-14-1649-2014.pdf>

Response: The paper is cited.

4. Page 19548, the word “numerous” is in my opinion not justified as Figure 7 shows one example of a laboratory measured phase function and one example of an in situ measured phase function. Please re-write accordingly.

Response: Indeed, only two cases are compared between the theoretical calculations and laboratory measurements. However, we state that “the ice cloud optical properties were obtained in numerous laboratories and field campaigns” by other researchers.

5. Page 19548, when discussing the PN I believe you are missing a number of Gayet et al. citations. Please include some of those citations in your manuscript.

Response: Two more papers by Gayet et al. on the subject are cited in the revised manuscript.

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6. Page 19549, line 1. All the citations are somewhat biased towards particular groups, what about work that has used the dual-viewing ATSR-2 instrument and multiple viewing MISR, for instance McFarlane et al citation should also be added here. Here are some suggestions. <http://onlinelibrary.wiley.com/doi/10.1029/2007JD009191/abstract>  
<http://onlinelibrary.wiley.com/doi/10.1029/1999JD900842/abstract>  
<http://www.opticsinfobase.org/ao/abstract.cfm?uri=AO-44-19-4060>

Response: These three papers are cited.

7. Page 19552. Lines 13-19. This argument is only true if the monomers making up the aggregate are sufficiently separated from each other so that multiple scattering between monomers is negligible. There might be instances where the constructions are such that the phase functions could be different between different realizations due to multiple scattering between monomers.

Response: We agree with the reviewer, and modified the relevant statement accordingly.

8. Page 19551, line 4. The authors state “..seldom does an cloud model...” This statement has been addressed by Baran et al. (2014), whom show that an ensemble model can indeed be consistently applied across the spectrum to simulate different measurements from the UV to radar frequencies, see <http://onlinelibrary.wiley.com/doi/10.1002/qj.2193/abstract>

Response: This paper is now cited in the revised manuscript. .

9. Page 19553, line 16. Please add in the discussion of surface roughness the Ulanowski et al. (2014) citation and also Ulanowski et al. (2006), which is found here [http://homepages.see.leeds.ac.uk/~lecsjed/huiyi/habit/habit\\_Aug\\_2011/papers/sdarticle%5B1%5D.pdf](http://homepages.see.leeds.ac.uk/~lecsjed/huiyi/habit/habit_Aug_2011/papers/sdarticle%5B1%5D.pdf)

Response: Both papers are cited now.

10. Page 19553, line 20, the application of surface roughness to the two component model is this applied to all sizes? If so, please state this or the size range over which it

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is applied.

Response: The surface roughness is applied for particles over the entire size range, and we clarify this point in the revised manuscript.

11. Page 19554, is the determination of in situ IWC based on the PSD integration assuming some mass-D relationship or bulk measurements of IWC? In the former case, is the exponent assumed in the mas-D relationship the same as the fractal dimension of your aggregate model?

Response: Instead of the mass-D relationship, we gave the volume-D relationship in the revised manuscript (see Eq. 4).

12. Following equation (1)  $D_{mm}$  should follow?

Response: Based on the definition of  $D_{mm}$ , it is only used as the upper/lower limits of the integrals in Eq. (2). It is unlikely to obtain an analytical expression in the form “ $D_{mm} = \text{something}$ ”. However,  $D_{mm}$  included in Eq. (2) can be obtained numerically.

13. Page 19555, line 3, “..is the density of ice”-> “..is the density of solid ice..” and please state the density of solid ice assumed.

Response: The definition is specified, and the value used is given.

14. Page 19555, line 8, the 11 field campaigns, the PSD measurements, what was the range of particle size measured? Was the maximum particle size  $< 1$  mm? This is important as it has implications for the effects of shattering on their dataset, see Korolev et al. (2013). Indeed, in this section the authors should state whether their datasets are affected by shattering or how shattering was minimized in their case.

Response: The details of the microphysical observations are not given in the manuscript. We use results from 11 field campaigns, and the information on them can be easily found in the relevant reference. Indeed, there are observations with small particles. In the revised manuscript, we discussed the different performance of

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the THM in modeling the microphysical properties at different bins of Dmm and IWC.

15. Page 19555, line 12 suggest replacing “under” by “colder than”

Response: Modified.

16. Page 19556, line 23, they use the term “solved”, their “solution” is not unique as a combination of microphysical models could be used to give similar results, I believe the word “solved” is not warranted. Please re-write this sentence accordingly. Indeed, the above paragraph lines 12-21 directly contradict the statement contained in section 4.

Response: The sentence is rewritten.

17. Page 19557 line 5, do they mean “increases” rather than “decreases” as particle size increases? (Auer and Veal, 1970).

Response: The model we used is the same as that used by Yang et al. (2013) and Bi et al. (2014), and the details can be found in the cited references.

18. Page 19557, section 4.1. I assume all single-scattering calculations are for random orientation? If so please state this.

Response: Random orientation condition is stated in the revised manuscript.

19. Page 19558, line 5, suggest “repeated” rather than “recaptured”.

Response: the suggestion is taken and revision is made accordingly.

20. Page 19558, line 29, how well does the value of the asymmetry parameter of the habit mixture model compare against observations?

Response: The values of the modeled and observed asymmetry parameters of the two cases are given in the revised manuscript.

21. Title section 4.2, no need for the word “the” in the section title.

Response: “The” has been deleted in the revised manuscript.

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22. Page 19559, line 15, there are exceptions to featureless phase functions at backscattering angles such as the cases discussed by Gayet et al. (2012) and Baran et al. (2012). <http://www.atmos-chem-phys.net/12/9355/2012/acp-12-9355-2012.pdf> and references therein and other studies.

Response: Actually, both papers were cited in the manuscript. In the revised manuscript, it is further clarified that the unusual scattering phase functions were observed from in situ measurements, while they are not considered for building the THM.

23. Page 19559, in the discussion of number concentration measured by the PN on line 21, the likely effect of shattering on this instrument should be discussed.

Response: For the case we use, Febvre et al. (2009) articulated that the effects of ice crystal shattering on the measurement is not very important, and, thus, they will not be considered in our study. We added the relevant discussion in the revised manuscript.

24. From Figure 8, what is the value of the phase function at 180° given by the two component model? And how does this value compare against the CALIOP observations given in Baum et al. (2011)?

Response: Considering the difficulty associated with accurate simulation of the phase function at 180° (especially for the geometric optics method), we didn't discuss the back scattering in this paper.

25. Page 19561, line 9, do you mean the far-infrared? In which case you should cite Cox et al. (2010) located here <http://onlinelibrary.wiley.com/doi/10.1002/qj.596/abstract;jsessionid=AA7EBE2992CB5F4DD67A9062F9BB574B.f01t03>

Response: The paper is cited.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19545, 2014.

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